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Interactive comment

# *Interactive comment on* "Evolution of events before and after the 17 June 2017 landslide at Karrat, West Greenland – a multidisciplinary approach for studying landslides in a remote arctic area" *by* Kristian Svennevig et al.

**Anonymous Referee #1**

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I read a multi-approach study on mass wasting activity in West Greenland, revolving around a major landslide that happened in summer 2017. The authors combine a large set of interesting, valuable, and complementary data sources to shed light onto the evolution and properties of an apparently notoriously active hillslope. I found the findings and benefits of combining the different sources of information intriguing in general and see a benefit in the approach itself. However, at the same time I think the study suffers from several, partly severe flaws that challenge a publication in its current shape.

At the current state, I see the general lack of a proper research question that would generate value and impact beyond just the case study scope. Currently, the article generates just incremental new knowledge. The workflow has already been presented in an earlier study. The main 2017 event itself and some of the secondary events have been described in detail by other studies (which have been adequately referenced). So, without describing a new method or workflow and without providing substantial original insight to previously reported events (apart from a few minor and indeed valuable prior and posteriour slope failures), the scope of the study shrinks to the two research questions the authors present at the end of the introduction: i) understand the processes that lead to 2017 event and ii) evaluate the risk of future events in the area. Sadly, in my opinion, these two research questions, despite being relevant and timely, cannot be be addressed with the methods and data the authors have presented. Neither the drivers nor the triggers of the 2017 event could be constrained with sufficient rigour, and also the trends of future activity are a matter of speculation. Investigating drivers and triggers would at least require additional data on for example meteorological, geophyiscal and ground properties of the area, none of which are presented. Constraining future areas at risk would need to go significantly beyond qualitative statements of potential rock texture characteristics or speculations about future permafrost trends. Thus, I recommend the authors develop one strong research question they can address with their data, and the data is indeed very promising, given that the analysis goes beyond the currently very descriptive nature (see below).

The description of methods is in many ways not rigorous enough to allow a solid judgement of whether or not the data can support the stated claims. As a few examples: There is no detail about the software and workflow used to perform the InSAR analysis. There is no information on the seismic data handling and analysis (e.g., signal preprocessing, detection of events, location of events, magnitude estimation, description/analysis of event signals; from the data presented in figure 5, it looks like the raw seismograms were inspected, without deconvolution, without filtering, without description of the spectral properties and their evolution, without inversion of the data for forces or other target variables). There is only very diffuse information on how optical remote sensing data was interpreted to identify features, no software or workflows are described. Thus, as a reviewer I mainly have to guess what might have been done. And especially for the first two points this is very unsatisfying and far from good practice. Thus, I strongly suggest the authors provide substantial detail on their workflows and software environments, to allow readers (and reviewers) to judge the validity and rigour of their analyses.

Similar to the above point, several results and conclusions appear not supported from the presented methods. Examples are bedding characteristics (constrained from geological map, own mapping, UAV data,...?), triggers of events, permafrost influences and trends, sliding plane angles (how evaluated, what are uncertainties, and so on), volume calculations (how constrained, how processed, what are the uncertainties). This does not mean I do not believe in the results, it just means the authors must give more detail about how the material they present has been generated to avoid such questions as above in the first place.

Overall, the presented material is very descriptive and subjective. The descriptive character is in itself not necessarily a flaw, but it is a pity that the authors did not go beyond that stage of data presentation, as the data would allow for much more quantitative and detailed insights, insights that would massively increase the impact of the study. InSAR data can yield so much more than just colourful pictures (without a legend by the way) and separating areas (of which size and with which degree of overlap to the failed sites?) of decorrelation from areas of coherence (by which measure actually?). Seismic data (see references of what other people have done with seismic data sets) can give so much more insight to the dynamics of mass wasting events (force inversion, volume estimates, duration and evolution,...). In summary, it appears that the authors merely scratch at the surface of their data, although digging a bit deeper would require just a bit more effort (or collaboration with experts in the respective fields). Thus, I recommend more detailed and derivative, quantitative analysis of the valuable data at hand, an analysis that shall be tightly aligned to the previously developed research question.

The subjective character refers to incomplete presentation of the data. As a Null hypothesis, I would state that there has been mass wasting activity all the time, and not just during the periods presented in table 2. To reject that Null hypothesis, a reader needs to be convinced that the data (mainly optical imagery, seismic waveforms, InSAR-based surface change) indeed shows no activity/change throughout the period covered by the data. What is the coherence like for all InSAR scences? Can mobility hotspots be identified, tracked and quantified through time? What does the seismic data say about hillslope-related activity since about 2000? How many seismic events were detected and located, and how? I strongly encourage the authors to put more effort in turning the qualitative data into appropriate quantitative data, which would allow more robust, rigorous and valuable insight to the dynamics of this very interesting study area.

In summary, I indeed believe there is valuable information and insight hidden in the manuscript. But at the current state this potential value is cluttered and diffuse, and the authors must put significant effort in sculpting the valuable information from the big chunks of raw data they are currently presenting. In addition, the study must develop a clear research question that can be pursued by the data and analyses applied to it. Given that these above points can be addressed in the near future, I would be very happy to give more detailed feedback on the methods, results and implications of the study.

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